Workfare - effective activation, pathway into the low-wage trap or both? Evidence from Germany

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Abstract

Workfare policies such as sanctions, workfare employment and counselling and monitoring schemes have been found to speed up labour market reintegration. At the same time, it has been suspected that this quicker reintegration is paid for with worse job quality, e.g. in terms of lower wages. I contribute to this discussion by analysing the effects of a workfare (counselling and monitoring) scheme from Germany on employment probability and post-unemployment wages via regression-adjusted matching estimations. This scheme tightens behavioural requirements for unemployed worker but also offers support in terms of more intense counselling. The results point to a strongly positive effect on employment probability but no effect on wages. They are robust to changes in the matching algorithm, and placebo tests refute concerns about endogenous selection or substitution effects. These findings contrast the results from previous research on sanctions, which confirmed a negative effect on job quality. This puzzle suggests that the existence of adverse effects on job quality depends on the type of workfare programme. While it may indeed be there for very intense kinds of workfare programme, it can be avoided if programmes find the right balance between pressuring and supportive components.

1. Introduction

Most post-industrialized countries have experienced an activating turn in social and labour market policy, which is characterized by a huge diversity of different activation policies (Eichhorst et al. 2008; Kenworthy 2010; Vlandas 2013; Haskins 2015). They range from long training programmes aimed at human capital accumulation to workfare policies which put a strong focus on quick reemployment. Unlike long raining programmes, workfare policies such as sanctions, public workfare employment as well as counselling and monitoring schemes have the advantage that they are non-expensive and easy to implement. Moreover, they are likely to yield quick returns because they have an immediate influence on targeted unemployed worker. From an economic and fiscal point of view, it is thus tempting to focus on workfare policies, especially in times of high unemployment and tight budget constraints (van der Klaauw and van Ours 2013; Andersen and Svarer 2014). In contrast, scholars of social and public policy have raised the concern that quicker, workfare-induced labour market integration is paid for with worse job quality, e.g in terms of lower wages (Barbier and Ludwig-Mayerhofer 2004; Taylor-Gooby 2004; Dingeldey 2007). If unemployed worker are pushed into the labour market by (almost) all means, it is possible that they are forced to apply for and accept available job offers which are worse than the best job they could have reached with less pressure but more support and time for job search. This raises the fundamental question whether workfare policies face a quantity-quality trade-off regarding their influence on labour market (re-) integration.

Exploring whether such a trade-off is present is of great importance in at least three regards. From the perspective of the individual, quicker but worse labour market integration is likely to result in lower levels of job and life satisfaction. From a more structural, political economy perspective on the labour market, low job quality of reintegrated worker bears the danger of increasing labour market dualization. In presence of a rapidly increasing gap between labour market insiders and outsiders, who experience frequent transitions between unemployment and unstable reemployment, high quality of labour market reintegration is of growing importance to keep outsiders from falling into a low-wage trap (Emmenegger et al. 2012; Schwander 2012; Fervers and Schwander 2015). Finally, from an economic point of view, it is questionable whether quicker bad worse labour market integration is beneficial in the long run, because worse job quality may incur human capital losses thus contributing to higher unemployment over the life course (van den Berg and Vikström 2014).

Despite the growing body of policy and programme evaluations in this field, evidence on the effect of workfare activation on job quality is very limited. I contribute to this discussion by analysing the effects of a large-scale workfare (counselling and monitoring) scheme from Germany on quantity and quality of labour market integration, measured by employment probability and post-unemployment wages, respectively. I combine administrative data from various sources to conduct different matching estimations with regression adjustment. The credibility of the analysis is enhanced by the high quality of the data, as well as the institutional setting of the programme which allows testing more rigorously for endogenous selection and substitution effects than many previous evaluations could.

The remainder of this paper is organized as follows. I start section 2 with some theoretical considerations on the expected effects of workfare programmes (2.1). Moreover, I review existent evidence on this issue (2.2). Subsequently, I present my empirical analysis in section 3. I start with a short description of the programme under discussion (3.1), which is followed by the explanation of the data sources and variables (3.2) as well as the identification strategy (3.3). Afterwards, I present and discuss the results of the treatment effect estimation including robustness and specification analyses (3.4). The last section (4) concludes with a short summary of the results and implications for future research and policy-making.

2 Workfare – quick but dirty integration?

2.1 Theoretical considerations

The starting point for the concern about a quality-quantity trade-off is the goal of workfare policies. They aim at quick reintegration, whereas job quality is regarded as less important, i.e. "emphasis is placed on the pressure or even compulsion for the unemployed (…) to (re-) enter the labour market, even with low-income-jobs (Dingeldey 2007:825)." This implies that the unemployed are encouraged or even enforced to accept "any job on the market as it is (Barbier and Ludwig-Meyerhofer 2004: 27)." The focus on quick reemployment may contribute to a quality-quantity trade-off for three reasons. First, they force unemployed worker to be less selective with regard to available job offers. If e.g. threatened with sanctions, the unemployed have no other choice but to apply for and accept any available job, even if there may be better job offers to come. Second and relatedly, the pressure of workfare programmes shortens the time that is available for job search. If the time for job search needed to find the best available job is, say, one year, but the workfare programme forces targeted unemployed worker to find and accept jobs within a shorter period of time, the reduction of time for job search will have adverse effects on job quality (Burdett 1979; Gangl 2006). Finally, and looking at this issue from a more sociological or social psychological perspective, workfare may lead to social stigma for targeted worker. Suffering from such a stigma may again lead to the acceptance of jobs which are worse than the best job that would have been found without this stigma (Wulfgramm 2014). All these three mechanisms can be expected to lead to quicker reintegration, but also to worse job quality. A similar argument has been outlined with regard to unemployment benefits. Benefits of short duration and low level influence the unemployed in a similar way workfare programmes do, they pressure them to accept available jobs quickly. Even though empirical evidence is somewhat mixed here, it shows that intense pressure on unemployed worker can have non-negligible negative effects on job quality (Gangl 2006; Tatsiramos 2009; Caliendo et al. 2013).

It has to be considered that these arguments generally apply for most kinds of workfare programmes, but not necessarily in the same way. Workfare programmes differ in their intensity and the mix of pressuring and supportive components. If a workfare programme entails severe sanctions after a very short period of time, the resulting positive (negative) effect on quantity (quality) of reemployment is likely to be very strong. In contrast, if a counselling and monitoring scheme increases the pressure on unemployed worker but also includes counselling services such as profiling or information about available job offers, the trade-off is likely to be much weaker. Therefore, a sound knowledge of the institutional setting of a workfare programme is not only of great relevance for developing and justifying reliable identification strategies, but also with regard to considerations concerning external validity. General conclusions should only be made with regard to programmes which are rather similar in their mix of pressure and support (and eventually also other institutional characteristics).

2.2 Previous Evidence

I briefly summarize existent evidence on the effects of workfare policies. Following the aforementioned argument, I distinguish between different kinds of workfare programmes, namely sanctions, counselling and monitoring programmes and public workfare employment.

Sanctions are probably the most intense kind of workfare. In their most extreme form, they withdraw any income from the unemployed, leaving them with very little choice concerning the compliance to their obligations. Previous research on sanctions has initially put a strong focus on the effect on the quantity of employment (measured by the probability of exit from unemployment or benefit receipt as well as outflow into employment). Overall, the results are quite optimistic. Positive impacts on one or more of these variables have been found in a number of studies for different countries, including Switzerland (Lalive et al. 2005), Netherlands (van den Berg et al. 2004; van der Klaauw and van Ours 2013) and Germany (Boockmann et al. 2014; Hillmann and Hohenleitner 2015). The magnitudes of the effects differ, but they are mostly reported to be very strong. For example, Lalive et al. (2005: 1404) estimate that the exit from unemployment increases by (everything else being equal) 25% if unemployed worker are threatened with sanctions, which is followed by another increase of 20% if the sanction is actually imposed. In sum, it has been concluded that sanctions are an effective means of increasing exit from unemployment and benefit dependency as well as reemployment probability (for a meta-analysis see also Kluve 2010). At the same time, this rather optimistic view has recently been challenged by empirical evidence which revealed a negative impact on job quality. Arni et al. (2013) rely on Swiss register data and detect a negative influence on post-unemployment wages and stability. Similarly, van den Berg and Vikström (2014) combine Swedish register data and information from a large-scale employer survey and confirm a negative effect on job quality in terms of wages, occupational level and the probability to move to a part-time job. These findings confirm the concern that quicker integration achieved by the means of sanctions is paid for with worse job quality.

Workfare employment and counselling and monitoring schemes are a less intense kind of workfare. Both kinds of programme may be accompanied with sanctions in case of non-compliance. At the same time, they also consist of supportive components. Counselling and monitoring schemes provide better information about available and suitable job offers, whereas workfare employment may support the unemployed to get used to regular working activities again. Therefore, one would expect a weaker effect on the quantity of employment but also less negative effects on job quality. Indeed, the results concerning the impact of counselling and monitoring schemes on labour market integration are more mixed. Neither Manning (2009) nor Ashenfelter et al. (2005) find any effect of counselling and monitoring schemes. In contrast, positive effects are reported by Graversen and van Ours (2008), McVicar (2010), Hägglund (2011) as well as Cockx and Dejemeppe (2012). Once again, the results differ between but also within studies. For example, the randomized experiment conducted by Hägglund (2011: 92) yields and increase in the outflow from unemployment (even before programme start) of about 50% in Jämtland, whereas the effect is insignificant for the three other Swedish counties. The estimates of Graversen and van Ours (2008: 2031) translate into a relative effect on the job finding rate of 30%, whereas McVicar (2010: 311) reports that the abandonment of counselling and monitoring has led to a 15% increase of registered unemployment. All in all, the effect on employment status has been reported to be either positive or insignificant. However, none of the aforementioned considers the impact on job quality.

The picture is similarly mixed for public workfare employment. On the one hand, the studies conducted by Huber et al. (2011) and Hohmeyer and Wolff (2012) both conclude that a large-scale public workfare programme from Germany (the so-called One-Euro-Jobs) does not foster labour market integration. On the other hand, Bennmarker et al. (2013) exploit a natural experiment from Sweden and estimate that the (threat) effect of a workfare programme on outflow from unemployment amounts to 10%. Their study is also one of the seldom ones which explicitly considers the quality of labour market reintegration. In contrast to the studies on sanctions, they do not find a negative effect on post-unemployment wages. This is consistent with the aforementioned argument that the expected quality-quantity trade-off is likely to be weaker. To sum up, previous research yields either positive or insignificant effects of counselling and monitoring schemes as well as public workfare employment on employment probability. At the same time, very little attention has been paid to the effect on quality of labour market reintegration, but the study of Bennmarker et al. (2013) gives a first hint that the trade-off for workfare employment is weaker than for sanctions.

All in all, it becomes obvious that further exploring the quality-quantity trade-off of workfare programmes remains a gap in the empirical literature. While additional research is needed for all three kinds of workfare programmes, the least is known for counselling and monitoring schemes. Therefore, I shed further light on this issue by presenting my empirical analysis on a counselling and monitoring scheme from Germany in the following section.

3 Empirical Evidence

I start this section with a description of the institutional features of the counselling and monitoring scheme under discussion. Afterwards, I describe the data sources and variables and explain my identification strategy. Finally, I present and discuss the results, including robustness and specification analyses.

3.1 Activating Citizens

Activating Citizens is a large scale counselling and monitoring programme from Germany. It started between July 2010 and June 2011 with 138,010 participants, who were scattered throughout the whole country. This makes it one of the largest ALMP programmes in Germany during this time. In addition to these basic facts, there are a couple of features which are of relevance for internal and external validity, namely the content of the programme as well as the mode of implementation.

While there are some differences in the administration of the programme between different regions, it essentially consists of more and intensive counselling services and monitoring of job search behaviour. This includes more frequent contacts between the targeted unemployed worker and its counsellor. Additionally, short courses such as learning how to write applications could have been part of the programme, but these courses are not aimed at systematic human capital accumulation but rather test the compliance of participants. If the unemployed do not comply with their legal obligations defined by the programme, they are threatened with sanctions in terms of benefit withdrawal. The period of increased counselling and monitoring (mostly) lasts for six months, and participation is mandatory in most job centres (the legal employment agencies). The goal pursued with the programme is direct labour market integration with nothing being specified on the type or quality of job. The target group was rather broadly defined, all participants who rely on social assistance benefits could have been selected as participants (receipt of social assistance in Germany mostly starts after a period of unemployment of at least one year but is then unlimited). For the identification strategy, it is crucial to note that the implementation mirrors the structure of a multi-level experiment (see e.g. Sinclair et al. 2012), i.e. there are participating and non-participating job centres, as well as participants and non-participants within participating job centres.

Taken together, these institutional features suggest that *Activating Citizens* is a comparatively soft workfare programme. There is no systematic human capital accumulation, the focus lies on quick reemployment, participation is mandatory, and non-compliance is sanctioned. However, these sanctions are only the last resort. The initial attempt of the programme is to reintegrate targeted worker by more intensive counselling. Therefore, the expected quantity-quality trade-off may still be present but likely to be weaker than for sanctions. Another aspect which makes the analysis of the programme particularly interesting is that the institutional features suggest a higher degree of external validity. Due to its large scale and the broadly defined target group, the programme is more representative for all unemployed worker than programmes which focus on a small and very selectively chosen part of the population. Moreover, the large scale implies particularly high policy relevance.

3.2 Data and Variables

I rely on register data to identify the effect of the programme, the Integrated Employment Biographies (IEB). This database contains daily information on all spells of all persons who are employed, unemployed, participate in an ALMP programme or receive social assistance. I have access to four subsamples of this database, one sample of treatment observations and three different samples of control units. The sample of treatment observations is a 50 % random sample of all participants, which amounts to 69,005 treated observations. They have all started programme participation between July 2010 and June 2011. The three samples of control units each consist of 125,000 observations. The first group of control observations consists of persons from participating job centres who would have been eligible for programme participation (i.e. have been unemployed and received social assistance at some point between July 2010 and June 2011) but did not actually participate. This sample will serve as the basis for the matching analysis. The two other samples will be used to identify substitution effects (see section 3.3). They both consist of individuals who have been or become unemployed between July 2008 and June 2009. One sample is drawn from job centres which have later participated in the programme, the other one is drawn from non-participating job centres.

Some sample restrictions have been imposed, but mainly for technical reasons. The only substantive restriction is that people had to older than 17 but younger than 60 years, because these groups of workers might be treated very differently by employment agencies. Moreover, observations have been discarded from the analysis if they have missing, strongly conflicting or unreliable information on very important covariates (e.g. gender) or on treatment information. For example, observations are discarded if their individual information indicates that they have participated in the programme, but they are administered by a job centre which does not participate in the programme at all. Even though the cleaning process leads to a loss of observations, the analysis can still rely on 63,707 treatment and 103,644 control observations.

Following my argument outlined in section 2.1, I use two dependent variables. To measure the quantity of reemployment I use a simple 0/1 indicator which is equal to one if someone is regularly employed and zero otherwise. Moreover, the dataset contains information on daily wages which are used as proxy for job quality. In the estimations for wages, I follow the approach by Bennmarker et al. (2013) who only rely on the wage information for persons who are actually employed (the implications of this measurement are discussed in section 3.4). Both variables are recorded in monthly intervals beginning from individual participation.[1] Additionally, the dataset consists of a rich set of covariates. Given that the literature offers no clear-cut criteria on which variables (not) to use for the matching analysis, the selection of covariates is mainly based on the experience of previous ALMP evaluations as well as theoretical considerations. To begin with, I use information on socio-demographic characteristics and household composition, namely age, sex, education, family status, German/non-German citizenship, health, size of the household, number of own children and total dependent children in the household, as well as the number of adult and unemployed persons. Moreover, I include information on lone parenthood. In addition to this standard information, the dataset contains two additional groups of variables which are particularly valuable for the matching analysis. First, several special characteristics are recorded by the employment agencies. These include the subjective assessment of future employment prospects by the counsellors from the employment agency, the reason for the end of the last spell of social assistance receipt, and whether someone has ever dropped out of an ALMP programme due to inappropriate behaviour or has finished an ALMP programme unsuccessfully. The latter two variables are of particular interest because they can be seen as proxies for usually unobservable variables such as motivation or behavioural characteristics. Second, I can rely on very detailed information on past employment history. This includes some characteristics of the last job, namely whether someone has worked as

[1] For programme participants, their individual start of the programme is the start of the measurement of the dependent variables. For non-participants, there is no actual start of non-participation. Therefore, a hypothetical programme start has

white-collar or blue-collar worker, the degree of complexity and the industrial sector. Moreover, I have extracted information on all spells of regular employment, subsidized employment, unemployment, and programme participation. I have calculated the number of months in the respective employment status within the first, second to fourth and fifth to seventh year before the official programme start. Additionally, I include a 0/1 indicator which states whether someone has ever been regularly employed during the last seven years. I refrain from the approach of Biewen et al. (2014) who match exactly on employment sequences of binary variables which indicate whether someone has been employed in a certain year, because the variance within these sequences is too low for my sample. Finally, I include some additional regional information from another administrative source on the local labour market situation at the job centre level. Since treatment and control observations both come from participating job centres, differences in regional variables only result from different distributions of participants and non-participants between job centres and are therefore rather small. Hence, I limit myself to the regional employment and unemployment rate as well as GDP per capita. All ordinal variables are split into dummy variables to avoid functional form misspecification in the propensity-score estimation.

Table A.1 gives the number of observations and the mean for both dependent variables and each covariate, separately for treatment and control observations. Checking for ex-ante covariate differences is crucial for the matching analysis, because strong differences may result in thin common support. This implies that (depending on the matching algorithm applied) that either many observations will be discarded from the analysis, or few observations receive very high weights and dominate the estimator (Imbens 2015). Therefore, it contributes to the reliability and robustness of the analysis that covariate differences are very limited. Moreover, it is worth mentioning that existing differences do not point to strong and systematic positive or negative selection even though there is a certain tendency for positive selection. On the one hand, participants are slightly higher educated and their labour market history is somewhat more favourable. On the other hand, the subjective assessment is worse and the incidence of lone parents is higher. Together with the high number of observations that is available, this creates very favourable conditions for the matching analysis. The raw data for wages and employment probabilities show limited differences, too, whereas participants have slightly lower integration rates but higher wages. However, this difference cannot be interpreted as causal effect. The picture may change if observable characteristics are conditioned on.

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been defined. It is equal to the start date of the programme plus a random variable which mirrors the temporal pattern of the inflow into the programme of participants between July 2010 and June 2011.

3.3 Identification Strategy

Following the potential outcome framework (Rosenbaum and Rubin, 1983), the treatment effect on the treated is equal to the outcome they have realized by participating in the programme, and the one they would have realized without participation. Given that the latter cannot be observed, it has to be estimated using a control group. Therefore, the treatment effect estimation is based on the comparison of participating and control observations. To begin with, I follow the most common approach in programme evaluation and apply a matching analysis. I use all variables described in section 3.2 as covariates. To decide about the details of the matching analysis, I rely on recent insights from the microeconometric treatment effect estimation literature on the finite sample properties and performance of different algorithms and approaches (for recent and sophisticated examples see Iacus et al. 2011; Abadie and Imbens 2006, 2011; Hainmueller 2011; Huber et al. 2014). Based on Monte-Carlo-Simulations, these and other studies have created some guidance on the performance of the estimators. No estimator dominates all the other ones, but there seem to be a couple of reasonable approaches. I decide to start with radius matching with regression adjustment as suggested by Huber et al. (2014). I follow some of the advice from their companion paper (Huber et al. 2013) when selecting tuning parameters. Therefore, I start with radius matching with linear bias correction, and the radius is defined as three times the maximum distance in propensity scores that would have been reached with one-to-one-matching. In the initial estimation, I do not restrict the maximum weight that is given to one particular observation, because I am confident that the favourable conditions will not lead to high weights for some observations, anyway. Nevertheless, these still somewhat arbitrary decisions should be subject to robustness checks. To begin with, I restrict the maximum weight that is given to one observation to four percent of total weights. This trimming procedure performs best in the Monte-Carlo Simulations of Huber et al. (2013). Afterwards, I replace radius matching with mahalanobis matching with regression adjustment as outlined by Abadie and Imbens (2006; 2011). Finally, I use inverse probability weighting with regression adjustment as alternative approach.

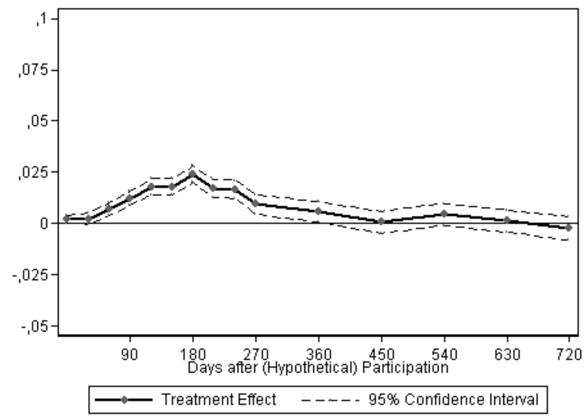
Even though the results will show that the matching analyses are very robust to changes in the algorithms and tuning parameters, there may be concerns about systematic bias due to violations of the identifying assumptions. The estimates can only be interpreted causally if the conditional independence assumption (CIA) and stable unit treatment value assumption (SUTVA) hold (see e.g. Keele 2015). The conditional independence assumption states that potential outcomes are (conditional on observable variables) independent of treatment status. This means that there must no unobserved differences between treatment and control group be left, which affect both the outcome and treatment assignment probability. In the given context, there is no clear-cut reason which points to endogenous selection. The treatment group is rather broadly defined and observable covariates do not point to strong selectivity. Moreover, the credibility of the CIA is enhanced by the exceptionally good quality of the data. Even though potentially relevant variables such as career preferences or motivation are not observable, it seems reasonable to argue that these have been absorbed by past employment outcomes or the information on behaviour in previous ALMP programmes. Nevertheless, I conduct a placebo-test on endogenous selection (Heckman and Hotz 1989; Imbens 2015; Imbens and Rubin 2015) to further substantiate the credibility of the CIA. This test is based on a matching estimation, in which a variable that is connected to the actual outcome variable but unaffected by the treatment is defined as the (placebo-) outcome. If the matching analysis reveals a significant effect on this pseudo-outcome, endogenous selection is likely to be present. In ALMP evaluations, past employment outcomes are natural candidates for the placebo-outcomes. Since I have used information of employment biographies of the past seven years (which should then obviously not be used as placebos), I define the number of months in employment eight and nine years before the start of the programme as placebo-outcomes. For sake of robustness, the matching analysis is conducted twice, with radius as well as mahalanobis matching.

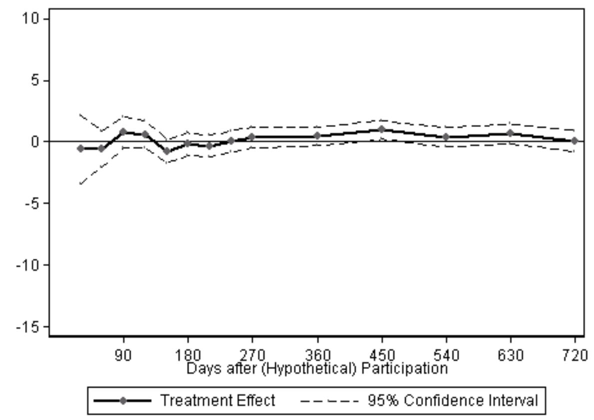
Finally, whether the stable unit treatment value assumption (SUTVA) holds is rather ambiguous from a theoretical point of view. As in any other ALMP programme, it is possible that treated worker simply substitute untreated ones, e.g. because they are better equipped for job interviews (Imbens and Wooldridge 2009). Moreover, the execution of the programme might lead to a redistribution of resources to the disadvantage of untreated worker in the same jobcentre, because participating job centres do not receive additional funding for the programme. Due to budget constraints, this is likely to result in reduction of time and effort spent for non-participants which could worsen their employment prospects. Taken together, both factors may lead to negative effects on non-participants, which would bias the matching estimation upwards. Such interferences between units have recently been a very active field of research in almost all disciplines that apply statistical methods. The gold standard for their estimation that has recently occurred is what Sinclair et al. (2012) refer to as multilevel experiments. In these multilevel-experiments, there are treated and untreated clusters (e.g. regions), and treated and untreated observations within treated clusters. Interference is then estimated by (regression-adjusted) difference-in-means comparisons between untreated observations from treated cluster, and untreated observations from untreated cluster (for applications from different theoretical contexts see Nickerson (2008), Ichino and Schündeln (2012), VanderWeele, Tchetgen and Halloran (2012), or Crépon et al. (2013)). A similar idea will be applied here: I observe non-participants from participating as well as non-participating job centres. The test for interference between units is therefore based on the comparison of the outcomes of these two groups. However, unlike in the aforementioned applications, it has to be considered that regional participation is not randomized. Therefore, raw differences in employment outcomes may also stem from regional selection bias, i.e. differences in regional labour market conditions or in the socio-demographic composition of unemployed worker. Due to the comparatively low number of observations at the upper level (the jobcentres), differences in regional characteristics will be difficult to balance in a matching analysis. Therefore, I combine matching with difference-in-differences estimation, i.e. DiD-estimation in a matched sample (semi-parametric DiD, Abadie 2005). To this end, I rely on the two samples of workers who have become unemployed between one and two years before the start of the programme (i.e. between July 2008 and June 2009). Untreated worker from participating job centres are defined as (pseudo-) treatment units, non-participants from non-participating job centres function as control units. One point in time within the period before the start of the programme (i.e. July 2008 to June 2010) is defined as t_0, whereas one point in time after the start of the programme (i.e. between July 2010 and June 2012) is defined as t_1. The time difference between both points in time is two years (e.g. January 2010 and January 2012), whereas I conduct the analysis for several points in time. For sake of robustness, I conduct this analysis twice, once with mahalanobis and once with radius matching.

3.4 Results and Discussion

The treatment effect estimations are summarized in graph 1. The left panel shows the results for integration into regular employment, the right panel the ones for wages at different points in time after programme start. The point estimates represent absolute (regression adjusted) differences between treatment and matched control group in the incidence of employed worker and daily wages, respectively. The effects on labour market integration refer to net integration rates, i.e. the corresponding outcome variable is coded 0 if someone is not employed at the respective point in time, regardless whether s/he has been in and out again of employment after programme start.

Labour market integration | **Post-unemployment wages**





Graph 1. Treatment effects based on radius matching with regression adjustment. Thin lines represent confidence intervals, thick lines are the point estimates. The left graph shows treatment effects with regard to labour market reintegration in percentage points, the right graph shows treatment effects on daily wages in €.

The effect on programme participation is positive from the very beginning and starts to accelerate towards the (scheduled) end of the counselling and monitoring scheme. It reaches up to 2.4 percentage points. The corresponding estimated potential outcome means of the treatment and control group are 9.4 and 7.0 percent which translates into a relative effect of 35 percent. Compared to the results reported from previous research, this is a rather strong effect but still close to e.g. the ones reported by Graversen and van Ours (2008). The treatment effect goes down after 180 days and reaches zero towards the end of the observation period. However, the effect after 180 days is (partly) a combined effect of *Activating Citizen's* and the subsequent PES. Given that this PES displays remarkably negative employment effects (IAW, ISG 2015), the effect of the counselling and monitoring scheme can be assumed to be positive in the long run. In any case, the effect on cumulated time in employment is even positive at the end of the observation period even for the combined effect, because the displayed effects are net integration rates. It can be concluded that the programme has fulfilled its purpose of fostering labour market integration of participants.

The decisive question now is whether this acceleration of labour market integration is paid for with worse job quality. Somewhat surprisingly, the estimated effects on wages of those who found employment are almost close to zero and clearly insignificant for all points in time. Even though the number of observations is lower than the one in the estimations for labour market integration, this does clearly not reflect a lack of statistical power as indicated by the very narrow confidence intervals (e.g. after 180 days, the number of integrated worker for who reliable wage information is available still amounts to 7,149 observations). Given that the average daily wages amount to about 36€ (once again depending on the point in time, note that not all persons are full-time employed), even the upper or lower bound of the confidence interval would translate into negligibly small relative effects. The conclusion that there is no adverse effect on job quality is further substantiated by the argument outlined by Bennmarker et al. (2013) who point out that in case of a positive effect on labour market integration, the estimates for wages of those who found employment represent a lower bound. The underlying assumption of this argument is that even if the CIA holds, we would expect that there are (possibly unobservable) differences within treatment and control group with regard to labour market attachment. Moreover, we would expect that persons with more favourable characteristics are integrated first. Therefore, a higher share of integrated worker within the treatment group implies that more persons with less favourable unobservable characteristics are included in the wage effect estimations than in the control group. This may result (if anything) in negatively biased results. Even though this possible bias is likely to be small in presence of high-quality data, this further supports the conclusion that there is no adverse effect on job quality.

Even though the quality-quantity trade-off was expected to be weaker, it is important to check the robustness and reliability of these somewhat surprising results both in methodological as well as substantive terms. From a substantive point of view, it may be argued that the absence of a quality-quantity trade-off is due to the target group. Even though it has been rather broadly defined, the descriptive statistics show that participants are characterized by rather low labour market attachment. In fact, almost half of the participants (and the control group) have not been regularly employed for the last seven years. It seems reasonable to argue that the outlined arguments which contribute to a quality-quantity trade-off rather apply for worker with higher labour market attachment, because after periods of unemployment of many years, it is questionable whether more time for job search is still beneficial. Therefore, I repeat the analysis but limit the sample to participants who have been employed in at least 12 months within the last four years. The results of this estimation are summarized in graph A.1. They reveal that the effect is not different for this subsample. Compared to the effects for the whole sample, the effect on labour market integration is somewhat weaker in the beginning but stronger at later points in time. The only remarkable difference is that absolute integration rates are higher in both groups (21.0 and 17.5% after 180 days), which is clearly consistent with the theoretical expectations. The effect on wages is again close to zero and insignificant. Given that the outlined distinction is somewhat arbitrary, I have also tried other sub-samples constructions such as the restriction of the sample to persons who have ever been employed within the last seven years (not shown, available upon request), but the results again rarely change. It is worth mentioning that the same holds true for other socio-demographic characteristics which are typical suspects for effect heterogeneity, namely age, gender, or region of residence (East vs. West Germany). Apparently, the programme effect does not vary systematically with indicators of employment history or other socio-demographic characteristics.

From a methodological point of view, it still has to be investigated whether the results are robust to methodological choices by the researcher, or whether all these matching estimations are systematically biased by endogenous selection or substitution effects. To test the robustness of the results, I have conducted the analysis with alternative estimation approaches as outlined in section 3.3. Graph A.3. summarizes these results. The upper left panel show the results from the original analysis again (to allow comparisons via a quick glance). The upper right corner replicates this analysis with the restriction on the maximum weight given to one observation. The lower part of the graph shows the results for inverse probability weighting (left) and mahalanobis matching (right) with regression adjustment. Once again, differences in the estimated effects are very limited indicating that the results are not sensitive to methodological choices. Finally, the specification analyses neither point to endogenous selection nor substitution effects. As graph A.3 shows, the estimated effects on both placebo-outcomes (months in employment eight and nine years before programme start) is close to zero and insignificant for both matching algorithms. It is worth mentioning that this is a remarkable finding given the high statistical power (exceptionally high number of observations) of this test. This implies that there is no indication for endogenous selection which confirms the claim that conditional independence is a reasonable assumption in presence of such high quality data. Similarly, the results shown in graph A.4 refute concerns about substitution effects. It displays estimated substitution effects for different points in time after programme start (e.g. the coefficient at 90 days after programme represents the effect when 90 days after programme start is t_1, and the point in time two years before that is t_0). For both estimations, the results are almost exactly zero for all points in time during the counselling and monitoring scheme. They get marginally significant towards the end of the observation period for mahalanobis matching but are very small in magnitude. In any case, this (if any) very small degree of interference does in no way affect the results from the matching analysis in substantive terms.

4 Summary and Conclusion

This paper has been motivated by the question of whether workfare activation policies face a quality-quantity trade-off in their effect on employment outcomes of targeted worker. It has been suspected that quicker, workfare-induced labour market integration is paid for with worse job quality. Despite a huge and growing body of policy and programme evaluations in this field, the effect of workfare policies on job quality has been considered only recently and remains an important gap in the literature. I have contributed to this discussion by analysing the effect of a workfare activation (counselling and monitoring) scheme from Germany. My results do not confirm the concern about a quality-quantity trade-off. The programme exerts a rather strongly positive effect on employment probability which reaches 35 percent towards the end of (scheduled) programme duration. At the same time, there is no effect on wages of those who have been successfully integrated into the labour market. These findings are robust to methodological changes (namely different matching algorithms or trimming procedures) and do not vary systematically with socio-demographic characteristics such as age, gender, region of residence or employment history. Moreover, specification analyses refute concerns about biases in the matching estimations due to endogenous selection and/or substitution effects.

These results are in line with the results on public workfare employment outlined by Bennmarker et al. (2013) but contrast the findings on sanctions, which indeed point to a quality-quantity trade-off of workfare policies (Arni et al. 2013; Van den Berg and Vikström 2014). Taken together, these findings reveal an interesting puzzle. On the one hand, workfare activation in its most extreme form (namely sanctions) indeed seems to lead to quicker integration of worse quality. On the other hand, quicker, workfare-induced integration does not automatically go along with adverse job quality. Apparently, it is possible to speed up labour market integration without worsening job quality if the right balance between pressuring and supportive elements is found. This seems to apply for the activation scheme under discussion which tightens the behavioural requirements for unemployed worker at the time offering support in terms of counselling services. This puzzle has two implications for future research. First, it reveals that previous categorizations of activation policies have been too broad. Distinguishing between "emancipating" activation which focusses on supporting unemployed worker (e.g. via long training programmes) and "repressive (Vlandas 2013:5)" workfare activation which forces them into the labour market by all means ignores the diversity of activation programmes within these two categories. Therefore, it should be an ongoing task for future research to develop more nuanced typologies of activation policies. Bonoli (2010) has made a first promising step into this direction. Second and relatedly, the question of which components or combinations of workfare policies may contribute to quicker labour market integration without hurting job quality needs further exploration. By analysing the effect of different workfare activation policies on quantity and quality of labour market integration, empirical research can constitute the basis for well-informed public policy-making that succeeds to reduce unemployment at the same time circumventing the danger of pushing unemployed worker into a low-wage trap. Only by relying on such an empirical basis, policy-makers can continuously improve public policies (Besharov 2009). In this regard, a lot of work in this area remains to be done.

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Appendix

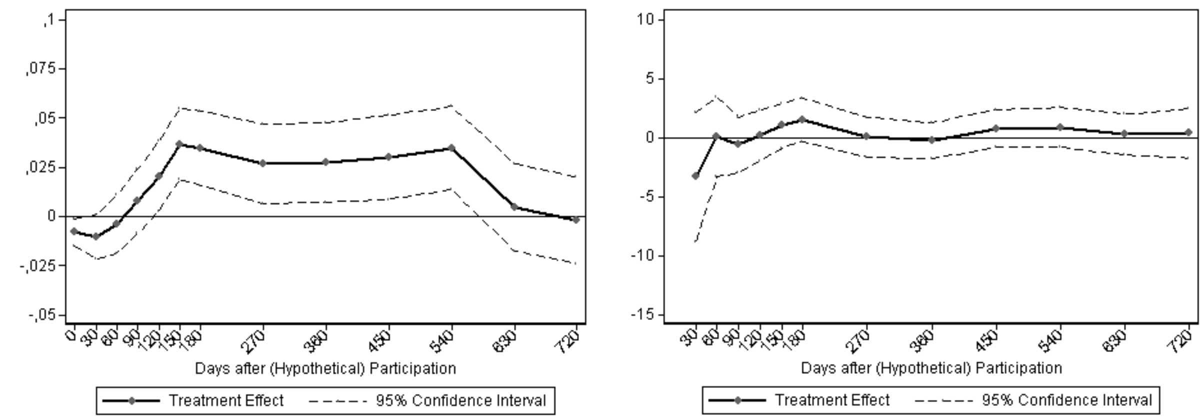
Variable	Participants		Non-Participants	
	N	Mean	N	Mean
Socio-demographics				
Employment probability 180 days after programme start	63878	0.10	103641	0.11
Daily wage 180 days after programme start (if employed)	2332	38.23	4817	36.94
Gender: female	63878	0.45	103641	0.46
No graduate	63877	0.10	103617	0.14
Intermediate school certificate, no professional qualification	63877	0.24	103617	0.29
High school degree or vocational training	63877	0.58	103617	0.47
High school degree and vocational training	63877	0.04	103617	0.05
Subject-linked university degree	63877	0.02	103617	0.02
University degree	63877	0.02	103617	0.03
Age group < 25 years	63878	0.04	103641	0.14
Age group 25 until 30 years	63878	0.13	103641	0.14
Age group 30 until 35 years	63878	0.13	103641	0.13
Age group 35 until 40 years	63878	0.14	103641	0.11
Age group 40 until 45 years	63878	0.18	103641	0.12
Age group 45 until 50 years	63878	0.20	103641	0.13
Age group 50 until 58 years	63878	0.15	103641	0.18
Age group > 58 years	63878	0.03	103641	0.05
Family Status: living alone	63878	0.45	103641	0.45
Family Status: married/living with a partner	63878	0.33	103641	0.34
Family Status: divorced/widowed/living separately	63878	0.23	103641	0.21
Family Status: missing	63878	0.00	103641	0.00
Citizenship: German	63878	0.90	103641	0.81
Citizenship: other than German	63878	0.10	103641	0.19
Citizenship: missing	63878	0.00	103641	0.00
Health problems: yes	63870	0.25	103520	0.22
Household information				
Child < 3 years: no	63878	0.58	103641	0.50
Child < 3 years: yes	63878	0.03	103641	0.04
Child < 3 years: missing	63878	0.39	103641	0.46
Child between 3 and 6 years: no	63878	0.58	103641	0.50
Child between 3 and 6 years: yes	63878	0.04	103641	0.04
Child between 3 and 6 years: missing	63878	0.39	103641	0.46
Child between 6 and 10 years: no	63878	0.54	103641	0.47
Child between 6 and 10 years: yes	63878	0.08	103641	0.07
Child between 6 and 10 years: missing	63878	0.39	103641	0.46
Child between 10 and 15 years: no	63878	0.52	103641	0.46
Child between 10 and 15 years: yes	63878	0.09	103641	0.08
Child between 10 and 15 years: missing	63878	0.39	103641	0.46
Role within household: Main person	63878	0.86	103641	0.79
Role of within household: partner	63878	0.13	103641	0.15

Role of within household: minor, unmarried child / unmarried adult	63878	0.01	103641	0.06
Role of within household: missing	63878	0.00	103641	0.00
Number of persons in BG: 1	63878	0.48	103641	0.46
Number of persons in BG: 2	63878	0.23	103641	0.23
Number of persons in BG: 3	63878	0.14	103641	0.15
Number of persons in BG: 4	63878	0.09	103641	0.10
Number of persons in BG: 5 or more	63878	0.06	103641	0.07
Number of persons in BG: missing	63878	0.00	103641	0.00
Number of persons of age able to work: 1	63878	0.62	103641	0.58
Number of persons of age able to work: 2	63878	0.31	103641	0.34
Number of persons of age able to work: > 2	63878	0.06	103641	0.08
Number of persons of age able to work: missing	63878	0.00	103641	0.00
Number of persons under age able to work: 0	63878	0.91	103641	0.91
Number of persons under age able to work: 1	63878	0.08	103641	0.08
Number of persons under age able to work: mehr als 1	63878	0.01	103641	0.01
Number of persons under age able to work: missing	63878	0.00	103641	0.00
Number of unemployed persons: 0	63878	0.98	103641	0.97
Number of unemployed persons: 1	63878	0.02	103641	0.03
Number of unemployed persons: > 1	63878	0.00	103641	0.00
Number of persons above age limit: 0	63878	1.00	103641	0.99
Number of persons above age limit: 1 or more	63878	0.00	103641	0.01
Number of persons above age limit: missing	63878	0.00	103641	0.00
Lone parent: no	63878	0.83	103641	0.86
Lone parent: yes	63878	0.17	103641	0.14
Additional administrative information				
Profile: Integrated	63878	0.07	103641	0.07
Profile: Market, activation, promotion	63878	0.16	103641	0.22
Profile: About to develop	63878	0.35	103641	0.26
Profile: About to be stable	63878	0.18	103641	0.14
Profile: Support necessary	63878	0.15	103641	0.15
Profile: missing	63878	0.09	103641	0.14
Job returner: no	63878	0.95	103641	0.96
Job returner: yes	63878	0.05	103641	0.03
Job returner: missing	63878	0.00	103641	0.01
Responsible administrative body: ARGE/gE [s.o., green area]	63574	0.93	103423	0.93
Responsible administrative body: gT/gAw	63574	0.07	103423	0.07
Responsible administrative body: zkT	63574	0.00	103423	0.00
Reason for end of receiving social assistance benefits: start of work	63878	0.07	103641	0.07
Reason for end of receiving social assistance benefits: relocation	63878	0.11	103641	0.13
Reason for end of receiving social assistance benefits: omission of employment	63878	0.13	103641	0.12
Reason for end of receiving social assistance benefits: other reasons	63878	0.16	103641	0.18
Reason for end of receiving social assistance benefits: missing	63878	0.53	103641	0.50
Special status	63878	0.15	103641	0.25

Relieved receiving of benefit: children	63878	0.02	103641	0.02
Relieved receiving of benefit: job returner	63878	0.05	103641	0.02
Relieved receiving of benefit: none	63878	0.85	103641	0.75
Relieved receiving of benefit: missing	63878	0.09	103641	0.21
Dropout of measure due to inappropriate behavior	63878	0.03	103641	0.02
Dropout of measure due to other reasons	63878	0.04	103641	0.03
Measure not completed successfully	63878	0.05	103641	0.03
Employment History				
Information on last job				
Blue-collar worker	63878	0.11	103641	0.12
White-collar Worker	63878	0.03	103641	0.03
profession: missing	63878	0.86	103641	0.85
Semi-skilled worker	63878	0.14	103641	0.16
Professionally oriented activities	63878	0.69	103641	0.64
Complex specialized activities	63878	0.07	103641	0.07
Highly complex activities	63878	0.08	103641	0.08
Complexity: missing	63878	0.02	103641	0.05
Manufacturing/ processing trade / agriculture	63878	0.41	103641	0.36
Service sector or others	63878	0.59	103641	0.64
Indicators of past employment history				
Number of months employed: 1 years before 2010	63878	0.36	103641	0.62
Number of months employed: 2-4 years before 2010	63878	2.53	103641	3.55
Number of months employed: 5-7 years before 2010	63878	4.13	103641	4.70
Number of months unemployed: 1 years before 2010	63878	9.47	103641	8.65
Number of months unemployed: 2-4 years before 2010	63878	25.34	103641	21.37
Number of months unemployed: 5-7 years before 2010	63878	14.93	103641	11.88
Number of months seeking work: 1 years before 2010	63878	0.83	103641	0.49
Number of months seeking work: 2-4 years before 2010	63878	1.86	103641	1.20
Number of months seeking work: 5-7 years before 2010	63878	1.28	103641	0.92
Number of months program: 1 years before 2010	63878	0.53	103641	0.66
Number of months program: 2-4 years before 2010	63878	1.87	103641	2.28
Number of months program: 5-7 years before 2010	63878	6.80	103641	4.76
Employed at all in the last 7 years before 2010	63878	0.44	103641	0.45
Regional information				
Regional unemployment rate (level of job centers)	62722	10.05	102493	9.86
Regional employment rate (level of job centers)	62722	50.32	102493	49.53
GDP per capita of employed person (level of job centers)	63149	54149.53	101594	59327.68

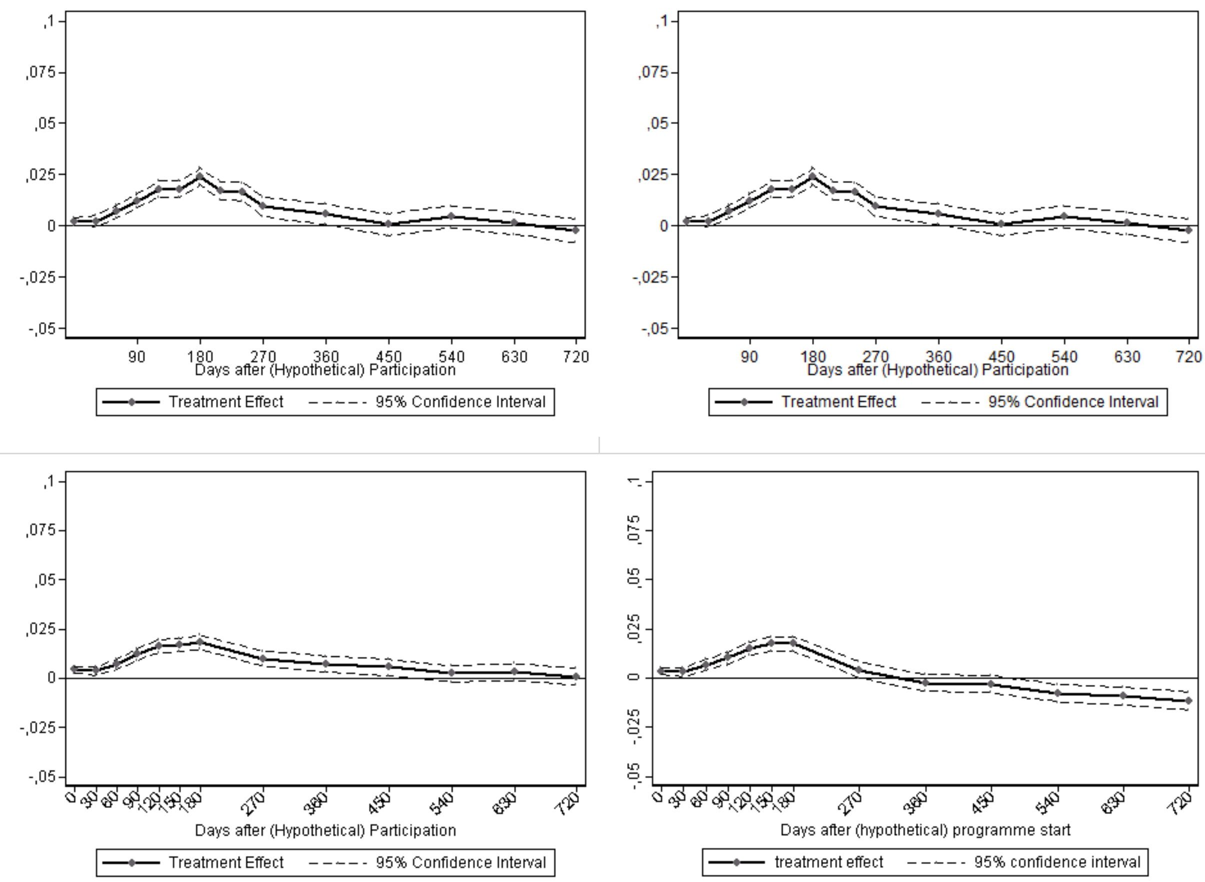
Table A.1. Summary of descriptive statistics for participants and non-participants. Source: own calculations

Labour Market Integration	Wages
Effects for participants with higher labour market attachment	



Graph A.1. Treatment Effects on worker who have been employed in at least 12 months within the last four years. Source: Own calculations based on IEB.

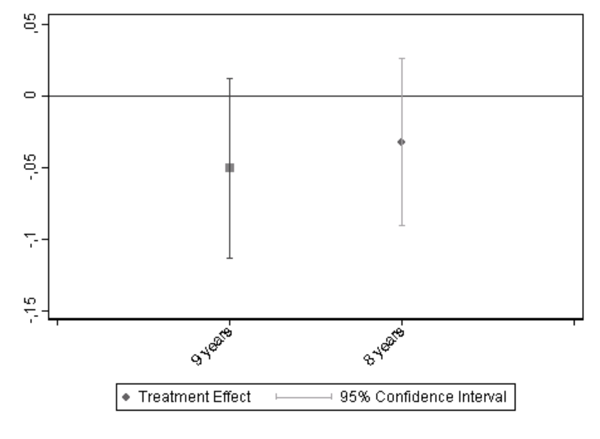
Inverse Probability Weighting	Mahalanobis Matching
Integration into regular employment	

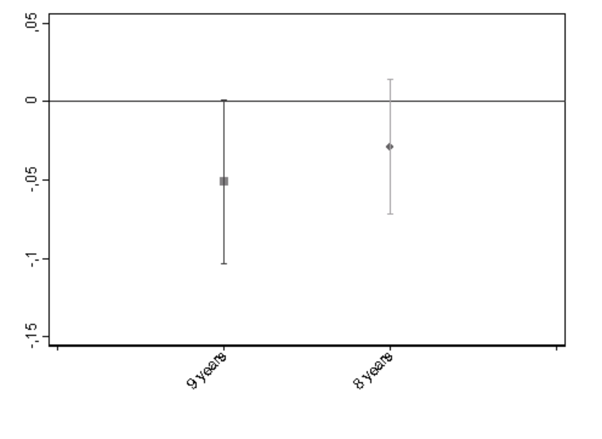


Graph A.2. Treatment Effects based on different matching/weighting estimations with regression adjustment. The upper left panel show the results from the original analysis again (as reference category). The upper right corner replicates this analysis with the matching procedure outlined in section 3.3. The lower part of the graph shows the results for inverse probability weighting (left) and mahalanobis matching (right) with regression adjustment. The Source: own calculations based on IEB.

Radius Matching	**Mahalanobis Matching**

Test for Endogenous Selection

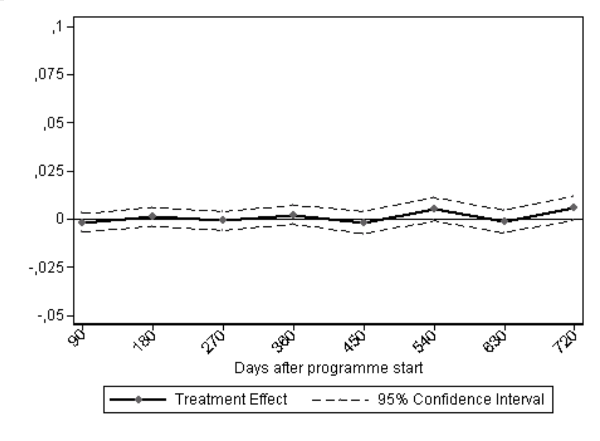


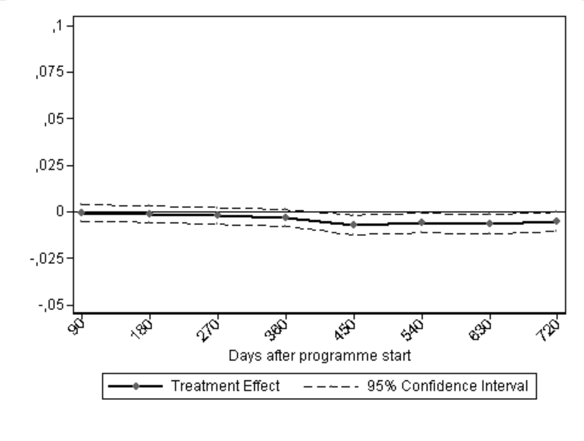


Graph A.3. Estimated (pseudo-) treatment effects based radius-matching (left) and mahalanobis-matching (right) with regression adjustment. Source: own calculations

Radius Matching	**Mahalanobis Matching**

Test for Substitution Effects





Graph A.4. Estimated substitution effects based on semi-parametric difference-in-differences estimation with radius-matching (left) and mahalanobis-matching (right). Source: own calculations based on IEB. The points in time refer to the definition of the point t_1, t_0 is the point in time two years ago.